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ABSTRACT

Seemingly conflicting points of view concerning language instruction which are expressed in various teaching methodologies are reconciled in this paper. Key issues discussed include: (1) the nature of linguistic rules and their relation to the "habits" of language use, (2) the role of grammatical theory in language teaching, (3) the nature of language learning, (4) a balance between an audiolingual habit theory and a cognitive code theory, and (5) some of the critical variables in language pedagogy. The author illustrates why the field of language instruction has become characterized by pedagogical uncertainty and concludes that the teacher's ability to manage learning behavior remains one of the most unexplored, unstudied variables in educational research. (RL)

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RESEARCH MEMORANDUM

RM-71-4

CURRENT ISSUES IN PSYCHOLINGUISTICS

AND SECOND LANGUAGE TEACHING

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U.S. DEPARTMENT OF HEALTH, EDUCATION & WELFARE
OFFICE OF EDUCATION

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Current Issues in Psycholinguistics and Second Language Teaching

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Introduction

The draft statement of Qualifications and Guidelines for Preparation of Teachers of English to Speakers of Other Languages now being prepared by a committee of TESOL makes reference at several points, quite properly, to the desirability of having the TESOL teacher adequately equipped with a basic knowledge of relevant disciplines. It is stated, for example, that the teacher "...should have insight into the processes of language acquisition as it concerns first and subsequent language learning and as it varies at different age levels." Later it is stated that the teacher "should acquire insight into the principles of educational psychology, linguistics, psycholinguistics, sociology and anthropology which he will use in facilitating the students' acquisition of the English language and of American culture."

As programmatic statements, these are acceptable and indeed laudable. The problem comes when one tries to implement them. In a review of the "state of the art" done for the Center for Applied Linguistics, Ronald Wardhaugh (1969) states that the theory of foreign language teaching is characterized by "uncertainty." He alludes to the "current ferment in those disciplines which underlie second language teaching, namely, linguistics, psychology, and pedagogy." This statement echoes that of Chomsky (1966), who characterized linguistics and psychology as being in a state of "flux and agitation"--a state to which he himself had contributed not a little. The journals in language teaching--particularly the International Review of Applied Linguistics and Language Learning (Michigan)--

are peppered with articles debating one point of view or another. First, it is suggested that transformational grammar has important insights for applied linguistics (James, 1969); then we are told that it is a complete failure in language teaching (Johnson, 1969; Lamendella, 1969). First we are told that pattern practice is without any scientific foundation, then we are told that pattern practice is indispensable and that there is no opposition between it and transformational grammar (or any other kind of grammar) after all (Brown, 1969). Some writers assume that teaching method is everything; others tell us that method makes no difference. In this bewildering interplay of diverse opinions and controversy, how is the language teacher going to be able to draw any firm conclusions about how to teach? What are the trainers of TESOL teachers going to tell them about linguistics, psychology, and pedagogy?

It has been some time since I have taken the opportunity to express myself on some of these questions, but in the interval I have also had the opportunity to rethink my position, sift the arguments of the various competing theories, and come up with what I hope may be a more balanced view of the issues.

Our field has been afflicted, I think, with many false dichotomies, irrelevant oppositions, weak conceptualizations, and neglect of the really critical issues and variables. When I summarized (Carroll, 1965) two extreme points of view in language teaching as being, first, the "audiolingual habit theory," and second, the "cognitive code-learning theory," I had no real intention of pitting one against the other. I was only interested in pursuing what each theory would imply if pushed to the limit. Indeed, even at that time I meant to suggest that each theory had a modicum of truth and that some synthesis needed to be worked out. Instead, the trend has been for points of view to become crystallized and polarized.

In this paper I want to suggest how the apparently conflicting points of view may be reconciled; I want also to suggest that the debates that have raged in language teaching theory may have entirely missed some of the truly essential points. We have been hung up on issues that turn on semantics or misunderstandings. Some of the traditional wisdom in language teaching continues to be valid. We ought not to "throw out the old man with the bilgewater." At the same time we must guard against a "new orthodoxy" that may lead us down a garden path to failure.

I will orient my talk around a number of key issues: the nature of linguistic rules and their relation to "habits" of language use; the role of grammatical theory in language teaching; the nature of language learning; the balance between an audiolingual habit theory and a cognitive code theory; and some of the critical variables in language pedagogy.

The False Opposition between "Rule-Governed Behavior" and "Habits"

In various writings, Chomsky has led an attack on what he claims to be "behavioristic" doctrines underlying older, "structuralist" views of language. In particular, he claims that language behavior cannot be conceived in terms of the notion of "habit"; rather, it must be thought of as "rule-governed behavior." In this attack, he seems to assume that "habits" are automatic sequences of responses to particular stimuli. A memorized sentence would be a "habit" for him. I remember that on one occasion, at a forum held at the Northeast Conference on the Teaching of Foreign Languages in 1966, he offered, as a kind of intuitive proof that language behavior is not a matter of habit, the fact that the first sentence of that day's New York Times had never been

written before--that it was entirely creative. As a psychologist, I cannot see that this is any proof at all. If we were to take that sentence from the New York Times and decompose it into its various constituents, even using a transformational grammar to find its phrase-markers and transformations, these constituents could be interpreted as manifestations of "habits" resulting from particular constellations of stimulus situations in the mind of the writer or in the material about which he was writing. What I am saying is that I do not find any basic opposition between conceiving of language behavior as resulting from the operation of "habits" and conceiving of it as "rule-governed." Perhaps it is simply that my conception of habit is different from Chomsky's. I would define a habit as any learned disposition to perceive, behave, or perform in a certain manner under specified circumstances. To the extent that an individual's language behavior conforms to the habits of the speech-community of which he is a member, we can say that his behavior is "rule-governed." For notice, what the descriptive linguist tries to do is to specify the manner and the circumstances under which certain classes of linguistic phenomena occur. Ordinarily, we think of these statements as rules, but they can equally well be thought of as statements of the conditions under which certain language habits manifest themselves in a given speech-community. As a matter of fact, linguistic rules are extremely limited; they specify only some of the conditions under which language phenomena occur; in general they take the form "if situation X is present (e.g., the necessity to place a certain concept in the subject-position in a sentence), the language form must be thus-and-so (e.g., the verb must be passive)." They do not attempt to formulate the

conditions under which the contingencies of the rule occur. At any rate, I believe that the opposition between "rule-governed behavior" and "habits" is false and specious.

I know that in saying this I subject myself to attack. I will be accused of not having read, or at any rate, not having understood, for example, Chomsky's (1959) famous review of Skinner's Verbal Behavior. To anyone who might thus accuse me, I would recommend the reading of a reply to this review by MacCorquodale (1970) in a recent issue of the Journal of the Experimental Analysis of Behavior. The complete defense of my point of view would require much more time than I can take here. There are a number of qualifications and details that would have to be discussed at length. Let me briefly mention some of these details.

In the first place I would emphasize that the notion of "habit" is much more fundamental, psychologically, than the notion of "rule." A "rule" is simply a formal, usually verbal, statement of the conditions under which something is expected to occur or not to occur, usually under certain sanctions. As such it is an abstraction or a construct in some sense independent of actual behavior. We are familiar with the fact that people can speak a language without any conscious knowledge or application of the "rules" that are said to underlie their language. A "habit," however, is a real thing that somehow resides in a person. It is what he has learned. As language teachers we are trying to produce and change certain kinds of habits. If psychology has anything to say to language teachers, it will say it about "habits," not about "rules," except possibly as second-order phenomena. You may look to linguistics for information about "rules."

Second, I would point out that there is not a one-to-one correspondence between linguistic habits and linguistic rules. There is only a partial overlap, consisting solely of those linguistic habits that are more or less uniform throughout a speech community and that consequently are legitimate objects of linguistic description. In the individual, there may be, and usually are, many idiosyncratic habits of speech that either have no functional significance in the speech community, or that are clearly at odds with those of the speech community. The individual may have a habit of using a word in a unique way. At the same time, many linguistic rules can be formulated that may not correspond to any functioning habit in the speech community. For example, rules have been formulated concerning the relations between the members of such pairs as sane--sanity, and vital--vitality, but if asked to pronounce a derivative in -ity from a nonsense form such as fane, many Ss will still say /feynity/, even though they will pronounce profanity in the usual way; they have no habit that reflects this rule, having learned sane and sanity, profane and profanity, etc. as separate items. In fact, the kinds of rules or habits they are really reflecting, in their behavior, are certain spelling-pronunciation rules, not rules relating words.

Third, I would point out that I do not exclude the notion of "knowledge" in discussing linguistic habits. The language user can become aware of his linguistic habits in various ways and in various degrees, and often he can report this knowledge. He "knows" the meanings of many words, and he can recognize when a word is used in an "incorrect" or unusual grammatical function. Modern psychology views the individual partly as an "information processor," and on occasion

the information he processes has to do with his linguistic habits. But there are undoubtedly a large number of linguistic habits that never rise to the level of conscious awareness for the ordinary individual. It seems to me to be an odd use of the verb to know when it is said that the linguistic system is what the individual "knows" in order to understand and produce grammatical sentences.

In this whole matter of the relation of psychology to linguistic theory, much of what has been said by the proponents of the new orthodoxy can be regarded as a kind of "verbal overlay" (to use a phrase coined by Lamendella, 1969) that has little real relevance to the facts of language behavior or even to the goals of linguistics.

One aspect of psychology that has been used as a whipping-boy is the so-called S-R (stimulus-response) theory. To be sure, there have been varieties of S-R theory whose application may be too limited, like Watson's or Skinner's, but from some linguistic writings one gets the impression that there is no such thing as a stimulus, and no such thing as a response. Actually, psychology cannot possibly dispense with these concepts, and they have been refined in various ways. The S-R formulation is usually expanded to include an "O" (for "organism") between the S and the R. The connection between an S and a response is seldom regarded as simple and automatic; it is modulated by the organism's past history, by other stimulus conditions, etc. The stimulus need not be external, either. We usually distinguish between the nominal stimulus and the functional stimulus; a nominal stimulus, for example, might be a grammatically ambiguous sentence in its purely physical manifestation (either written or spoken), while the functional stimulus would be what the hearer or reader perceives it to mean. Moreover, the

psychologist does not think of a response only in terms of a consequence of some specific stimulus. Nor does the response have to be defined in terms of a particular physical movement--saying a word, for example, for many purposes can be thought of as equivalent to writing the word. Finally, the concepts of stimulus and response are not inextricably bound to the concept of reinforcement. Rather, psychologists are more generally concerned with the outcomes of a particular S-O-R sequence and its effects on subsequent sequences. One kind of outcome has to do with an individual's knowledge of the outcome. Current opinion is that knowledge of outcomes is a much more effective factor in learning than "reinforcement" of some particular response. This is in line with the view of the learner as an "information processor" which is implied by nearly all the current work on human learning, and sometimes even in animal learning. Still, there appear to be certain classes of behaviors in which reinforcement theory is useful.

What seems to have happened is that because of Chomsky's attack on a particular variety of behavioristic psychology, and because of the unquestioning acceptance of this attack, linguists and language teachers have overgeneralized his conclusions to all of psychology and its concepts. Even some psychologists have been hoodwinked.

One consequence of the mistaken rejection of the concept of the stimulus has been to underplay the role of the objective situation and the environment in the formulation of linguistic rules. For example, linguists have formulated rules purporting to govern the relations among different types of sentences such as active, passive, negative, and interrogatives, but they have neglected to state rules concerning the conditions under which these types of sentences are used

and have meaning. To the claim that linguists often make that their goal is simply to determine the rules by which sentences are "grammatical" or "ungrammatical," I would suggest that one aspect of the grammaticality of a sentence may be whether its use is appropriate in a given situation. If I asked someone to open the door and he replied "Paris is the capital of France," I would ordinarily take his reply to be ungrammatical. It is in fact widely recognized that linguists have neglected the "semantic" component of language. The formulation of semantic rules will at some point entail a consideration of the stimulus configurations involved in them, because meanings have connections with stimuli and situations.

For example, it is impossible to write, in the usual linguistic manner, rules about the proper use of the definite and indefinite articles in English. One must make an appeal to the communicative situation--to the perceptions and intentions of speaker and hearer. To explain the fact that a speaker can use the indefinite article a (as in "I saw a movie last night") even when he has a perfectly definite thing in mind, we have to note that the speaker realizes that for the listener, this thing is still nonspecific when first mentioned. However, this introductory use of the indefinite article permits either the speaker or hearer to assume that the thing mentioned is now specified and thenceforth to use the definite article (e.g., if the speaker continues "The movie was about Russia" or if the listener asks "Did you like the movie?"). We can say in this case that the speaker's perception of the listener's state of knowledge is the functional stimulus to his choice of article. However, I suspect that extremely few speakers of English are aware of this "rule" or the

habit that corresponds to it. I wonder how many TESOL teachers are aware of it when they try to teach the use of indefinite and definite articles.

In a recent essay in Language Learning, T. G. Brown (1969) found himself in the embarrassing position of having to say that the concepts of pattern, habit, and interference were "conceptually inadequate" even though they related to what he felt were very real problems in teaching languages. Obviously Brown was under the influence of the "new orthodoxy" in linguistics. Brown had no need, in my opinion, to apologize for his concepts of pattern, habit, and interference. With appropriate definition, they are valid concepts. A pattern is a manifestation of a linguistic rule, usually, a particular surface structure, or it can be thought of as a manifestation of a linguistic habit. One strong, well-learned linguistic habit can interfere with the acquisition of another habit when there are certain similarities on either the stimulus or the response side of the habit.

What Grammatical Theory Is Most Useful to Language Teachers?

It would be pretentious to try to express an opinion here on which of the various grammatical theories is most valid. Different grammatical theories have somewhat different goals. The several varieties of transformational grammar are indeed quite successful in achieving their goals, although I have reservations about certain features of them, in particular, their frequent confusion of grammaticality with meaning. But lack of time forbids a discussion of this problem. In general I believe that language teachers should evaluate grammatical theories in terms of the degree to which they conform to the linguistic habits that actually enable a language user

to speak and understand the language. In the terms proposed by Chomsky, this would have to include both a theory of competence and a theory of performance. In fact, the theory of performance would be of importance at least equal to that of a theory of competence. I am not even sure that it will ultimately be possible to differentiate these two types of theory, although one can in general accept the usual distinction between *langue* and *parole*. The transformational theory of competence as it has developed thus far presents itself as mainly a set of abstractions that may or may not relate to actual linguistic behavior--*even though* it may be fairly successful in being "descriptively adequate." I fail to see how a pure theory of competence can have what Chomsky calls "explanatory adequacy" unless it includes at least part of a theory of performance. The evidence that transformational rules correspond to any habits that are actually involved in the behavior of speakers and hearers is thus far meager and highly controversial. For example, the fact that subjects seem to have slower comprehension of passive sentences does not need to be accounted for by appeal to transformational rules; it can be accounted for, possibly, by pointing out that passive sentences are less frequent in language use and hence less familiar to subjects in psychological experiments. The fact that they are ordinarily learned somewhat later than active sentences is possibly a factor, also the fact that subjects seem to possess an "expectancy" for active sentences.

Whether it would be in a theory of competence or in a theory of performance, or some combination thereof, an adequate theory would include the statement of rules (corresponding to habits, along the lines I have mentioned earlier), having to do with how speakers encode meanings into communicatively acceptable utterances, and how hearers decode

those meanings. One might suppose that transformational grammar is attempting to do this by specifying relations between "deep" and "surface" structure, if one assumes that "deep structure" is tantamount to "meaning to be encoded, or meaning to be decoded into." It is not clear, however, whether meaning is in fact encoded in the deep structure forms specified by transformational grammar. My prize example would be the famous sentence, "John is easy to please." Intuitively I would guess that the meaning being represented here is an attribute of John, "easy-to-please-ness" rather than some deep structure which we might verbalize as "For someone to please John is easy." Another example: if I know only that someone's windshield has been smashed, it seems to me that the meaning doesn't necessarily entail any reference to who or what smashed the windshield, or what it was smashed with. That it got smashed, under some indefinite circumstances, is all that is meant.

One aspect of grammatical theory that I believe has been somewhat neglected by linguistic theoreticians and that, if developed, would be of considerable use in language teaching, is the full description of the lexical and grammatical information associated with each of the words in the language--or at least the more common words. I have recently been studying the relevance of this information to the interpretation and comprehension of English sentences by native speakers at grades 3, 6, and 9 in school. I directed my attention in particular to words in English that, at least in their dictionary entry forms, can have multiple grammatical functions. Some of these words, like LEAN or SKIRT, have somewhat different meanings in their several grammatical functions; others, like FILL, SIGHT, CHANNEL, DRUG, have essentially the same meaning in different grammatical functions.

I collected a large number of such words with multiple grammatical functions and asked my subjects to use them in sentences. A striking result of this part of the study was that in most cases, subjects had strong tendencies to use each word in a particular part of speech, even though if asked to use the word "in another way" they would sometimes use it in another part of speech. For example, the word MILL was used 100% of the time as a noun by those 3rd graders who could use it correctly at all, and 89% of the time by 6th graders. Only 12% of the 3rd graders, and 25% of the 6th graders, changed part of speech in giving a second sentence. Furthermore, 76% of the 3rd graders, and 70% of the 6th graders, marked "wrong" the usage of the word in the sentence "Before class, children often mill in the halls," although of course most of the children (71% of 3rd graders, 92% of 6th graders) marked it "right" in the sentence "The children walked to the mill near the river."

From such findings, I conclude that a component of a child's competence in the English language is his "knowledge" of the lexicogrammatical information associated with each word--knowledge that could also be regarded as corresponding to a series of "habits" of various strengths to perceive the word in one or another part of speech and apprehend an associated meaning. I also conclude that for a large number of words, this lexico-grammatical knowledge is seriously defective in children as compared to that of the educated native speaker, and that this deficiency is possibly a serious and little recognized source of children's difficulty in comprehending language. Incidentally, this may be an interesting case of habit interference. A child's habit of understanding the word MILL as a noun appears to interfere with his comprehension when it is used as a verb.

I should think that information of this sort would be of interest and use to TESOL teachers. (It is contained in a report that I have submitted to the U.S. Office of Education and that will presumably show up in the ERIC system shortly.)

On Language Learning

The "new orthodoxy" in linguistics and psycholinguistics has made certain statements that may have made second language teachers almost despair of their profession. I refer to the claim that the acquisition of a first language depends to a great extent on some "innate" language ability whereby language is "acquired," not learned, by some mysterious process of "hypothesis testing." Certain writers go so far as to suggest that some ground-form of natural language is, so to speak, "wired in" to the human brain. It is also suggested that there is a critical period for language acquisition that lasts only until about the age of puberty, with in fact a decline of language learning ability from about the age of 5 or 6 up to puberty. It is true that these writers carefully leave open the possibility that people can learn a second language after puberty, but they suggest that second-language learning is different in kind from first-language learning.

What may make second language teachers despair is that the process of language acquisition, whether the first language or another, is depicted as so mysterious, and so different from ordinary learning, that they could never hope to compete with these processes.

In some measure, we may agree with this new orthodoxy. We can grant that there is indeed a large biological component in first language acquisition--that even though chimpanzees have been taught impressive language-like performances, those performances are nothing like those of

the average human child. We can grant that the human child has a very complex and well-differentiated brain and that language is acquired in step with the maturing of that brain. The evidence for a "critical period" and a decline in language acquisition ability during the middle school years is not strong, however, and even if there is some decline I am not persuaded that one must appeal to biology to explain it. An alternative hypothesis about this decline is that it is due to the consolidation of the habits established in primary language acquisition and their interference with the acquisition of new habits. Further, it may be that the large individual differences in foreign language aptitude that can be observed reflect individual differences in the rate of this decline.

My chief concern about the claims of the new orthodoxy, however, is that it underplays the role of learning, learning of the kind we know something about. It must be the case that in some sense children "learn" their language by observation, modeling, imitation, and similar processes. In fact, there is accumulating evidence that children learn language, at least in part, by processes that resemble those that can be studied in the laboratory, and that in some respects they are "taught" their language. It has been observed, for example, that mothers tend to simplify their language when speaking to their young children.

Common observation would suggest that people can and do "learn" second languages by normal learning processes--not necessarily by "reinforcement" procedures, but more likely by the conscious acquisition of knowledge about the language--knowledge that with time and appropriate experience is converted into what I have tried to call habits. I mentioned earlier that psychologists have come to understand better the respective roles of "reinforcement" and "knowledge of outcomes." Reinforcement techniques are mainly valuable in maintaining attention and interest;

they are the basis of successful programs of "behavior management" as practiced in some kinds of institutions, such as those for delinquents and the mentally retarded, and even in some regular school programs. But the current interest of psychologists studying human learning is in the investigation of basic processes of memory and concept formation. These studies assume that the individual has the capacity to perceive language stimuli, to "register" them in memory in some way--initially in what is called short-term memory and later in a long-term memory storage--and to "process" the information thus registered in order to make inferences about its nature and structure or in order to make new, seemingly "creative" responses in conformity with those inferences. They have been studying the factors that convert short-term memories into long-term memories, and the factors that enable the individual to derive inferences about his perceptions. For example, "rehearsal" (e.g., repeating words to oneself) is one of the processes that appears to convert short-term memories into longer-term memories. It is out of a psychology of this sort that I believe a psychology of language learning and use can be fashioned. In fact, some of the elements of such a psychology have been lying around for a long time.

Audiolingual vs. Cognitive-Code Learning?

From all of the above, one can see that I do not believe that either a pure "audiolingual habit theory" or a pure "cognitive code-learning theory" is a correct and comprehensive one. Each of these theories is to some extent wrong or incomplete. Yet each theory has elements of truth and value. In saying this I am not simply being "eclectic." Instead, I am trying to suggest a meaningful synthesis. Just what name we should use for a synthesized theory, I am not sure. If it does not seem too flip

to do so, let us call it a "cognitive habit-formation theory." To some, such a title may seem a contradiction in terms, but as we have already seen, the concepts of cognition and of habit-formation can be accommodated to each other.

Let us look at the merits and demerits of the theories that we want to synthesize.

In thinking about the audiolingual habit theory, we should first note that an incidental aspect of this theory was that it directed attention to a particular objective of language teaching, namely the aural-oral objective. This was a salutary emphasis, but it had little to do with language learning theory *per se*. If we are concerned with habits, it wouldn't matter whether they were formed in a spoken or written mode. Studies such as that of Scherer and Wertheimer (1964) have adequately laid to rest the idea that there is any marked advantage in starting with aural-oral teaching when a reading objective is to be attained. As I have commented elsewhere (Carroll, 1966) the Scherer-Wertheimer study shows mainly that students learn (if anything) precisely what they are taught, or at least that transfer of learning is a two-way street between aural-oral and reading-writing skills.

From the standpoint of methodology as such, the emphasis of the audiolingual habit theory was upon the formation of habits through practice and repetition. It re-introduced the pattern-practice drill that has been the bane of so many students and teachers. Now, language behavior *is* partly a matter of habits--habits of perceiving, knowing, and performance. What was wrong with the audiolingual habit theory was its incorrect assumptions about what kinds of habits to form and how to form them. It assumed that the habits that had to be formed related, for example, to the substitution of words in slots in sentence patterns or to the transformation of

one kind of sentence into another--habits that have only a remote relation to those that function in actual language behavior. If it had paid more attention to the formation of truly functional habits, it might have been more successful. As a simple example of a functional habit for a learner of English, one might cite a "habit" of starting the main clause with an auxiliary when the stimulus is the intention to ask a yes-no question based on any verb except the copula (and in some cases have). There is a subtle but important difference between this and the ability to transform a declarative sentence into an interrogative: the difference resides in the stimulus--situational and intentional in the former case, merely a sentence in the latter case. Even the memorization of dialogues, with their implied situational content, does not insure that the situation or the intention of the speaker will become a functional stimulus in the habits that are formed. The dialogue may be learned simply as a series of chained responses, one sentence being a stimulus for the next. It is only when the student has a strategy of injecting meaningful situational content into the dialogue that he may indeed profit from dialogue memorization. At least, this would be my analysis of such procedures; unfortunately I know of no hard evidence about this point.

On the formation of habits, the audiolingual theory assumed that practice and repetition were the crucial factors. But it was found long ago in psychology that practice and repetition are not crucial in learning, although they have certain roles to play. Successive repetition of the same response is, in fact, generally the wrong way to "stamp in" a habit; there are few kinds of learning where this is effective. Evocation of the response on a number of aperiodic, widely-spaced occasions, with interpolation of different material in the intervals, is a much more effective method of strengthening a habit, but such a method has been insufficiently employed in pattern drills.

Another important principle of learning that was often ignored in methods based on the audiolingual theory was the role of feedback and correction, or "trial and error," in its nonpejorative sense. In the language laboratory, students were too often permitted to repeat errors and thus to "learn" them, or if they were corrected, there was insufficient attention to evoking the correct response on another occasion or to explaining the nature of the error and how to correct it.

Now consider the cognitive-code-learning theory. It does have the merit of trying to give the student a knowledge and comprehension of the facts and formal rules of the language--a knowledge that can indeed be of help in guiding the formation of appropriate language habits, but only when appropriate opportunities are given to form those habits along the lines I have just mentioned. Not only was there the danger that a cognitive code-learning procedure failed to provide enough of these opportunities, but there was also the possibility that the facts of the language were presented in inappropriate or hard-to-understand ways. For example, the presentation of a complete verb paradigm is a dubious way of helping the student to form morphological habits because the task of reproducing a paradigm has little resemblance to anything in actual language use. (I don't mean to rule out all use of verb paradigms; I am simply suggesting that presenting a verb paradigm is not sufficient to produce the learning desired.)

The presentation of the "facts of the language" must take account of the student's readiness to absorb them; also, it must be done with an awareness that a variety of concrete examples must be given to illustrate and reinforce abstract rules. Research on inductive vs. deductive teaching methods indicates that neither method alone is adequate; for effective teaching, there must be considerable alternation between rules and examples. It

hardly matters whether one starts with the rule or the example, as long as this alternation exists.

Contrastive linguistics and error analyses based on it can play a definite role both in the organization and preparation of language teaching materials and in the day-to-day presentation of "language facts" to the student. But presentation of contrastive facts is best done, according to the kind of synthetic theory I am elaborating here, in the context of statements like "When you want to express meaning X, you do it in manner A, but when you want to express meaning Y, you do it in manner B," emphasizing the stimulus conditions that control the different usages rather than merely pointing out a contrast between language forms.

In the teaching of phonology, a cognitive habit-formation theory would recommend: "Do as much explanation and coaching as you can as to how the foreign sounds are to be recognized, discriminated, and articulated, but at the same time keep shaping the responses by feedback, correction, and practice procedures." Relevant research studies supporting this recommendation are now available from work of Henning (1966) and Catford and Pisoni (1970).

Pedagogy and the Teacher

Much of what I have said about teaching procedures is in no way new. The kinds of things that I believe a cognitive habit formation theory recommends be practiced have long been the property of good language teachers, from the days of Gouin, de Sauzé, Palmer, Sweet and other pioneers. Teachers need to be constantly reminded of these practices, however, because they tend to develop, under the pressure of new fads and theories, a kind of professional panic and anxiety about their work. Also, it is easy to neglect certain practices, such as giving proper feedback and

correction to every student, when there are too many students or when there are too many distractions of an administrative character.

In language teaching, as in other kinds of instruction, probably the critical factor in success is in managing the learning procedures of the student in such a way that at any given stage of learning the student is learning just what he needs to learn, being given the appropriate strategy for that learning to take place, and being properly reinforced in that learning. Any extreme, one-sided theory of language teaching tends to distract the teacher from his task and make him neglectful of certain essential operations in teaching. This is perhaps one of the reasons why comparisons between different teaching methods and procedures are seldom productive of any large average differences favoring one method or another. In the "Pennsylvania study" conducted and reported by Smith (1970), a large component of the variance was associated with the teacher, quite apart from method or material. I have reason to believe that this teacher variance reflected the extent to which the teacher was able to manage and maintain appropriate student learning behavior along the lines I have indicated--even though this was a variable that was never directly studied or observed in the Pennsylvania investigation. In fact, even though there has been considerable research on "teacher behavior," that research has focused on such things as "classroom climate" and student-teacher interaction patterns, largely neglecting the teacher's ability to manage learning behavior. Thus, the teacher's ability to manage learning behavior remains one of the most unexplored, unstudied variables in educational research.

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